PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (**http://bmjopen.bmj.com/site/about/resources/checklist.pdf**) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	The correlation of sarcopenia and depressive mood in older community dwellers: a cross-sectional observational study in china
AUTHORS	chen, lei; sheng, yunlu; qi, hanmei; tang, tingting; yu, jing; lv, shan

VERSION 1 – REVIEW

REVIEWER	Hiroyuki Umegaki Nagoya University
REVIEW RETURNED	Nagoya University 04-Mar-2020

GENERAL COMMENTS	I appreciate to have a chance to read the manuscript. The study investigated the association between depressive mood and sarcopenia in Chinese community-dwelling older people. The issue is interesting enough, however, the study has several important shortcomings. 1. The authors repeatedly referred to anti-depressive medications in Introduction and Discussion, however, many of the subjects involved were within normal range of GDS scores, and no indications to anti-depressive medications. 2. The first paragraph of the Discussion should summarize the results of the study. 3. I presume the subjects involved were Asian (it should be clearly stated in methods section). If so, the authors may want to use Asian Working Group for Sarcopenia (AGWS) criteria for sarcopenia (just recently revised). 4. The most important flaws in the study was lack of adjustment of physical activity as the authors stated. Because depressive mood tends to reduce physical activity to impair muscle power. I am very sorry to say that it is critical flaw in this study.

REVIEWER	Heewon Jung Seoul National University Hospital, South Korea
REVIEW RETURNED	12-Mar-2020

GENERAL COMMENTS	I read the manuscript by Lei et al, studying correlation between sarcopenia and depression in a cross-sectional of community dwelling Chinese older adult with great interest. I found that the study is well designed and analyzed appropriately on relationships between multiple facets of sarcopenia phenotype and depression spectrum with a commonly used scale (GDS), albeit with some weaknesses that authors also addressed well in the manuscript. However, I noted that there are still rooms for improvement for the manuscript with some notable points.

	1. Introduction - I think we can improve introduction part by dealing with the reason of the study - Why sarcopenia and depressive mood?. With existing evidence that physical activity can improve depressive mood in older adults, I think that we can start the intro firstly with clinical importance of sarcopenia-frailty in older adults. And then, we can remark that physical exercise is a well-known effective intervention for sarcopenia/frailty (enormous evidence out there, such as Fiatarone et al, NEJM, 1994), and we can connect these knowledge with depressive mood that is also somehow amenable by physical activity. Then authors can introduce depression thing and describe why studying the relationship between sarcopenia and depression. 2. Methods & results – It would be better to have some more details on protocols for physical performance measures (that how gaitspeed and SPPB components were measured in the study, for example, was gaitspeed measured with 3m- length vs. 4m- length in SPPB?, was gaitspeed measured with separate acceleration/deceleration length or standing-start manner?). Also, descriptions on blood works are missing in method section. 3. Results - On the other hand, I think correlations between glucose and lipid findings and physical performance measures are somehow out of focus of the study hypothesis, and not used further for mechanism seeking for main results. It would be better to drop these data (if so, there would be no need to describe blood works in method section). 4. Results - Is there any questionnaire data of physical activity in the study, even a brief questionnaire such as IPAQ? As authors digging into shared risk factors of depression and sarcopenia, I think further analyzing physical inactivity / sarcopenia spectrum / depression status would be interesting for the study. (Although authors already remarked in the limitation part that there seems no physical activity measure in the study, I think only a simple scale would be much helpful.) 5. Limitation – Mean SPPB scores of male/female in the study were 11.36 / 11.47 – meaning that study population is really ‘healthy’. Therefore, this might be a substantial weakness in terms of generalizability of the study. It would be better to have addresses some selection bias issue of the study in limitation part. 6. Using words - I hope authors to use words somehow more carefully in describing clinical works on aging population. Elderly □ older adults (see Vaughan et al, 2019, AGS Report on Engagement Related to the NIH Inclusion Across), subjects □ participants (since the study is observational study with the help of study participants). Also, with the present study focusing social and health issues rather than mere biological processes of aging, I suggest to use word set of age/men/women than sex/male/female.

VERSION 1 – AUTHOR RESPONSE

Reviewer(s)' Comments to Author:

Reviewer: 1
Reviewer Name
Hiroyuki Umegaki

Institution and Country
Nagoya University

Please leave your comments for the authors below
I appreciate to have a chance to read the manuscript.
The study investigated the association between depressive mood and sarcopenia in Chinese community-dwelling older people.
The issue is interesting enough, however, the study has several important shortcomings.
We would like to thank the reviewer for the encouraging words. We have endeavored to satisfactorily meet recommendations of revisions.
1.The authors repeatedly referred to anti-depressive medications in Introduction and Discussion, however, many of the subjects involved were within normal range of GDS scores, and no indications to anti-depressive medications.
We appreciate the reviewer's opinions. We agree that many of the participants involved were within normal range of GDS scores and antidepressants are indeed irrelevant to our article. We are sorry for the confused description and have removed the sentence in question and improved the introduction and discussion of our manuscript.

2.The first paragraph of the Discussion should summarize the results of the study.
Thank you for the kind reminder. We have rewritten the first paragraph of the discussion section to better highlight our results to the field as follows：“Overall, our data revealed an association between depressive mood and sarcopenia in adults aged 60 and over. This was consistent with the results of a meta-analysis, whose authors concluded that patients with sarcopenia were likely to present with depression18. Furthermore, we found that depressive mood was negatively associated with the strength of leg muscles and physical performance measured by gait speed and the standing balance and SPPB scores, even after adjusting for confounding factors. However, no significant correlation was observed between muscle mass and depressive mood in either men or women.”.

3.I presume the subjects involved were Asian (it should be clearly stated in methods section). If so, the authors may want to use Asian Working Group for Sarcopenia (AGWS) criteria for sarcopenia (just recently revised).
We appreciate the reviewer's opinion. We have amended the criteria for sarcopenia in the methods as follows: “According to the Asian Working Group for Sarcopenia (AWGS) criteria in Older People17, sarcopenia is defined according to muscle mass, muscle strength, and physical performance. Possible sarcopenia is determined by low muscle strength or low physical performance (5-repeat chair stand test≥12s). Sarcopenia is defined as low muscle mass plus either diminished muscle strength or low physical performance.
Low muscle mass was defined as an SMI below 7.0 kg/m2 for men and 5.4 kg/m2 for women. Low muscle strength was defined as handgrip strength < 28 kg for men and < 18 kg for women. Low physical performance was defined as gait speed<1.0m/s or an SPPB score ≤ 9”.
Meanwhile, we have also divided the participants into three groups according to Asian Working Group for Sarcopenia (AWGS) criteria for sarcopenia and conducted the statistical analysis as shown in Fig.4.

4.The most important flaws in the study was lack of adjustment of physical activity as the authors stated. Because depressive mood tends to reduce physical activity to impair muscle power. I am very sorry to say that it is critical flaw in this study.
We thank the reviewer for their opinion on our manuscript. We believe that being able to add physical activity is beneficial to analyze the risk factors of depression and sarcopenia. Thus, we do appreciate your viewpoint and will consider data of physical activity even a brief questionnaire such as IPAQ in future studies. We have also made the limitation clearer in the discussion as follows: “since better physical function is associated with improvement of sarcopenia and a lower incidence of depressive

symptoms, we believe that being able to add physical activity is beneficial to analyze the risk factors for depression and sarcopenia. We will acquire data on activity levels, such as with the International Physical Activity Questionnaire (IPAQ), in future studies."

Reviewer: 2
BMJ Open uses compulsory open peer review. Your name and institution will be returned to the authors and will be published with this review if the article is accepted.
By submitting your review you agree to the peer review terms and conditions. If the article is published, your name and review will also be published as supplementary information to the article. This means the review will be made available under the same Creative Commons license granted to the article.
Reviewer Name
Heewon Jung
Institution and Country
Seoul National University Hospital, South Korea

Please leave your comments for the authors below
I read the manuscript by Lei et al, studying correlation between sarcopenia and depression in a cross-sectional of community dwelling Chinese older adult with great interest. I found that the study is well designed and analyzed appropriately on relationships between multiple facets of sarcopenia phenotype and depression spectrum with a commonly used scale (GDS), albeit with some weaknesses that authors also addressed well in the manuscript. However, I noted that there are still rooms for improvement for the manuscript with some notable points.
We would like to thank the reviewer for the encouraging words. We have endeavored to satisfactorily meet recommendations of revisions.
1. Introduction - I think we can improve introduction part by dealing with the reason of the study - Why sarcopenia and depressive mood?. With existing evidence that physical activity can improve depressive mood in older adults, I think that we can start the intro firstly with clinical importance of sarcopenia-frailty in older adults. And then, we can remark that physical exercise is a well-known effective intervention for sarcopenia/frailty (enormous evidence out there, such as Fiatarone et al, NEJM, 1994), and we can connect these knowledge with depressive mood that is also somehow amenable by physical activity. Then authors can introduce depression thing and describe why studying the relationship between sarcopenia and depression.
We would like to thank the reviewer for the constructive suggestions. We have accepted the suggestions and read more references about physical activity, sarcopenia and depressive mood. Meanwhile, we have amended and improved introduction to make our aims "Why sarcopenia and depressive mood?" more reasonable.

2. Methods & results – It would be better to have some more details on protocols for physical performance measures (that how gaitspeed and SPPB components were measured in the study, for example, was gaitspeed measured with 3m- length vs. 4m- length in SPPB?, was gaitspeed measured with separate acceleration/deceleration length or standing-start manner?). Also, descriptions on blood works are missing in method section.
We appreciate the reviewer's suggestion and have added more details of SPPB components in our methods section as follows: "The chair-stand test, also called the chair-rise test, can be used as a proxy to assess the strength of the leg muscles (quadriceps muscle group). The participants were required to rise five times from a seated position as fast as possible without using his or her arms. The time was recorded manually with a stopwatch.
Physical performance was assessed by gait speed and the standing balance test16. For the gait speed, participants were asked to walk along a straight walkway on a flat floor at their usual speed without deceleration. They walked over a 4m distance between markers placed at 3 m and 7 m from the start of the walkway. The time was measured manually with a stopwatch, and then the mean

walking speed (m/s) was calculated. The test was performed twice, with the faster of the two walks used for analysis. For the standing balance test, participants were asked to stand in three positions (with feet together, with the inside of the heel of the front foot close to the big toe of the rear foot, and with one foot forward and one backward), using arms or other means to maintain balance without moving the feet.
The Short Physical Performance Battery (SPPB) comprises the measurements of gait speed, standing balance, and the chair-stand test. The maximum individual test score is 4 and the maximum total SPPB score is 12."
We are sorry for not having described blood works. However, we followed the reviewer's third suggestion, there would be no need to describe it in our revised version.

3. Results - On the other hand, I think correlations between glucose and lipid findings and physical performance measures are somehow out of focus of the study hypothesis, and not used further for mechanism seeking for main results. It would be better to drop these data (if so, there would be no need to describe blood works in method section).
We would like to thank the reviewer for this comment. We have removed all the data about blood glucose and lipid.

4. Results - Is there any questionnaire data of physical activity in the study, even a brief questionnaire such as IPAQ? As authors digging into shared risk factors of depression and sarcopenia, I think further analyzing physical inactivity / sarcopenia spectrum / depression status would be interesting for the study. (Although authors already remarked in the limitation part that there seems no physical activity measure in the study, I think only a simple scale would be much helpful.)
Thank you for identifying this limitation. We believe that being able to add physical activity is beneficial to analyze the risk factors of depression and sarcopenia. Thus, we do appreciate your viewpoint and will consider data of physical activity even a brief questionnaire such as IPAQ in future studies. We have also made the limitation clearer in the discussion as follows: "since better physical function is associated with improvement of sarcopenia and a lower incidence of depressive symptoms, we believe that being able to add physical activity is beneficial to analyze the risk factors for depression and sarcopenia. We will acquire data on activity levels, such as with the International Physical Activity Questionnaire (IPAQ), in future studies."

5. Limitation – Mean SPPB scores of male/female in the study were 11.36 / 11.47 – meaning that study population is really 'healthy'. Therefore, this might be a substantial weakness in terms of generalizability of the study. It would be better to have addresses some selection bias issue of the study in limitation part.
We appreciate the reviewer's opinion. We have accepted this as a limitation of our study and have highlighted the issue of selection bias in our discussion as follows: "Second, the mean SPPB scores of males/females in the study were 11.36/11.47, which means that the enrolled participants were relatively healthy. It is necessary to recruit more participants with large variability in their parameters to avoid selection bias."

6. Using words - I hope authors to use words somehow more carefully in describing clinical works on aging population. Elderly older adults (see Vaughan et al, 2019, AGS Report on Engagement Related to the NIH Inclusion Across), subjects participants (since the study is observational study with the help of study participants). Also, with the present study focusing social and health issues rather than mere biological processes of aging, I suggest to use word set of age/men/women than sex/male/female.
We would like to thank the reviewer for the constructive suggestions. We are sorry for our inaccurate description. We have amended the description "older adults/participants/men/women" instead of "Elderly/subjects/male/female" to better reflect the social and health issues of aging population.

VERSION 2 – REVIEW

REVIEWER	Heewon Jung Seoul National University Hospital, South Korea
REVIEW RETURNED	16-May-2020

GENERAL COMMENTS	Generally, the manuscript has been substantially improved accordingly to the reviewer's points, and I appreciate authors' effort in the revision process. A minor point - I found that although ethical standards were described, IRB number is missing in methods. For consistent ethical standards for the journal, I hope authors to provide IRB# for the study.